

probability of the occurrence within a given time of a given amount of deviation from the most probable distribution of a finite number of molecules of two different kinds in a vessel, and has given a numerical example of a particular case of the diffusion of gases.

The same method might be extended to the diffusion of heat by conduction, and the diffusion of motion by internal friction, which are also processes by which energy is dissipated in consequence of the motions and encounters of the molecules of the system.

The tendency of these motions and encounters is in general towards a definite state, in which there is an equilibrium of exchanges of the molecules and their momenta and energies between the different parts of the system.

If we restrict our attention to any one molecule of the system, we shall find its motion changing at every encounter in a most irregular manner.

If we go on to consider a finite number of molecules, even if the system to which they belong contains an infinite number, the average properties of this group, though subject to smaller variations than those of a single molecule, are still every now and then deviating very considerably from the theoretical mean of the whole system, because the molecules which form the group do not submit their procedure as individuals to the laws which prescribe the behaviour of the average or mean molecule.

Hence the second law of thermodynamics is continually being violated, and that to a considerable extent, in any sufficiently small group of molecules belonging to a real body. As the number of molecules in the group is increased, the deviations from the mean of the whole become smaller and less frequent; and when the number is increased till the group includes a sensible portion of the body, the probability of a measurable variation from the mean occurring in a finite number of years becomes so small that it may be regarded as practically an impossibility.

This calculation belongs of course to molecular theory and not to pure thermodynamics, but it shows that we have reason for believing the truth of the second law to be of the nature of a strong probability, which, though it falls short of certainty by less than any assignable quantity, is not an absolute certainty.

Several attempts have been made to deduce the second law from purely dynamical principles, such as Hamilton's principle, and without the introduction of any element of probability. If we are right in what has been said above, no deduction of this kind, however apparently satisfactory, can be a sufficient explanation of the second law. Indeed some of them have already indicated their unsoundness by leading to determinations of physical quantities which have no existence, such as the periodic time of the alternations of the volume of particular gases.<sup>1</sup>

J. CLERK MAXWELL

#### OUR BOOK SHELF

*Heroes of North African Discovery.* By N. D'Anvers. (London: Marcus Ward and Co., 1877.)

MR. D'ANVERS has here made an interesting *résumé* of

<sup>1</sup> See *Szil. Phil. Mag.*, October, 1876; Clausius, *Pogg. Ann.*, cxlii., p. 433; *Pogg. Ann.*, cxlvi., p. 585, May, 1872; J. J. Müller, *Pogg. Ann.*, clii., p. 125.

the work of the principal travellers who have made Africa known to the world. He briefly dismisses the earlier explorers, the bulk of the volume being devoted to those of the eighteenth and nineteenth centuries. Mr. D'Anvers has evidently read his authorities carefully, and gives a clear account of his heroes' adventures, and of the main results achieved. The book is evidently meant for young readers, and to them both the text and the numerous illustrations will prove attractive. But all who wish to have a fair knowledge of what has been hitherto achieved in the field of African discovery should read this interesting and instructive volume. The author prefixes a list of the authorities he has consulted, and promises another volume on South Africa, in which the results obtained by Mr. Stanley will be embodied.

*Manual of Agriculture; including the Application thereto of Chemistry, Geology, Botany, Animal Physiology, and Meteorology.* By Richard Henderson.

THIS is a reprint of one of the Highland Agricultural Society's prize essays. It forms a very marked exception from the thoroughly practical essays which are usually published by that society, so much so indeed, that it is a source of regret that a society which has done so much to improve agricultural education, should have in any way stamped the present work with its approval and authority.

The work is divided into seven chapters, of which five are devoted to some notices of chemistry, geology, botany, animal physiology, and meteorology, and the seventh alone treats upon the application of these sciences to agricultural practice, which is the professed subject of the work.

A few extracts from the first six chapters will give an idea of the character of this part of the work. The second chapter deals with chemistry, and is largely made up of comments upon eighteen elements, the descriptions being remarkably similar to those given by Roscoe in his "Lessons." It is fair to say that the author occasionally introduces original remarks, as, for instance, in saying that "carbon forms about fifty per cent. of the residue of plant-life when the latter is charred, and access of atmospheric air or oxygen prevented, for oxidised carbon escapes as a gas." Prof. Roscoe fares rather badly at the hands of our author, since he in another place says, "Roscoe gives the following graphic formula as the average composition of blood," and he appends the average percentage composition.

We are told again that at the sea-level the pressure of the air "can support a column of mercury thirty inches high in a tube *in vacuo*." Concerning fogs and mists, "they result from the radiation of heat from land and water, taking with it aqueous vapour, which becomes visible upon encountering cooler air. Similarly rain is produced when heated volumes of air are deprived of their heat, through the fall of condensed vapour, which assumes, according to the temperature it encounters, the form of hail, rain, or snow."

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

#### Sun-Spots and Terrestrial Magnetism

MR. B. G. JENKINS, in his letter to NATURE, vol. xvii. p. 260, says, "I have ventured to state my belief that we are now passing through a long minimum period, one very similar to that which occurred at the close of last century." It was the chief object of

my communication to NATURE, vol. xvii. p. 183, to show the latter fact as far as the observations go up to the present time. I did not, however, venture in that article to make a guess as to the future, which really could have little weight till we have another year's observations. Yet I thought it desirable, nearly two months ago, to place in the hands of Prof. Balfour Stewart the evidence that the possibility of such an event, as an obvious conclusion from my results, had not escaped me. As Mr. Jenkins has published his guess, I may do the same with mine. The latter differs, however, from his in a very important way. He supposes the next maximum will be in 1887, whereas I suppose that the weak maximum of 1797 may be repeated near 1880. In this latter case the interval between the two will be nearly double that found by me (forty-two years), during which the sun-spot period appears to have all its different lengths.

February 1

JOHN ALLAN BROWN

### Terrestrial Magnetism

I HEREWITH submit a notice of an experiment for illustrating to a class the action of terrestrial magnetism. In a simple way it clearly exhibits to a large audience the action of the currents of electricity that pass around the earth. The experiment was suggested on reading a paper by Prof. J. W. Mallet, F.R.S., of the University of Virginia, on "The Apparent Alteration in Weight of a Wire placed East and West, and Traversed by an Electric Current" (*Phil. Mag.*, November, 1877).

Instead of disconnecting the wires placed east and west from the portion of the rectangle, as was done by Prof. Mallet in the experiment alluded to, whereby the attractive or repulsive action of the earth currents on one side only of the rectangle was obtained, it occurred to me to suspend the whole rectangle to a balance. Properly arranged in this way the attraction for parallel currents in same direction, and repulsion for currents in opposite direction would generate a couple, tending to produce rotation around an east and west horizontal axis, and hence augment the deflection of the balance.

A rectangular frame was made of light poplar wood, of section three by two centimetres, whose sides were one metre in length by three-fourths of a metre in breadth. About the perimeter of this rectangle there were wrapped twenty coils of insulated copper wire. Each extremity of the wire was made to terminate near the centre of one of the shorter sides, and passing through the wooden frame, was fastened and cut off about 3 cm. from the frame.

This rectangular frame was then so suspended, in a horizontal position, by wires attached to the pans of an ordinary Delenil's hydrostatic balance, that the longer sides were at right-angles to the beam. By adjusting weights in the scale pans the index of the balance was brought to the zero. Two small orifices bored in a block of wood a centimetre apart, served as mercury cups, in which the extremities of the short terminal wires were immersed; near the bottom and through the walls of these wooden mercury-cups were screened small brass hooks, which served as connectors, to which the wires of the battery were attached. The balance was now so placed that the longer sides of the suspended rectangle were at right-angles with the magnetic meridian, or in the magnetic east and west line.

When the current from the battery was made to pass around the rectangle from east to west, on the northern side, and from west to east, on the southern side, by the theory of terrestrial magnetism, the north side of the rectangle would be attracted to the earth, and the south side repelled, and that this was so the corresponding deflection of the balance rendered plainly visible. When the current was reversed the deflection was in the opposite direction. By breaking and closing the circuit at proper intervals to augment the oscillations, the large frame was readily made to oscillate through an arc of 5°. When the sides of the rectangle were placed north-east and south-west, the current produced no sensible effect. A bichromate of potash battery of sixteen cells was used with plates of zinc and carbon 25 cm. by 6 cm.

With a rectangle containing a larger number of coils of wire attached to a delicate balance by the use of a constant battery, the variations in the earth's magnetism might be thus observed.

WM. LEROY BROWN

Vanderbilt University, Nashville, Tenn., January 11

### Seiches and Earthquakes

IN the last number of NATURE (p. 234) you make an allusion to the fact that the earthquake of October 8, 1877, has not been

traced by the self-registering "limnimeters" (tide-gauge) of M. Ph. Plantamour at Geneva, and myself at Morges. Let me take the opportunity of the great publicity of your excellent paper to ask the naturalists who live in other countries more frequently visited by earthquakes, for an explanation.

I believe I have demonstrated in many different papers that the phenomenon called *seiches*, which consists in rhythmical movements of the level of the lake, is a balancing-wave, a wave of stationary uninodal oscillation. The water moves in balancing oscillation in the two principal diameters of the lake, in the direction of the greatest length and of the greatest breadth. For setting the water in such an oscillatory movement there are two possible causes:—

1. A shock given to the water itself is the most frequent case, and I can prove that generally the *seiches* are caused by some rupture of the equilibrium of the atmospheric pressure; many storms, and especially those that fall somewhat abruptly on the lake, are accompanied by very high *seiches*, and I have many examples of the beginning of the oscillatory movement of the water exactly at the same time the storm commences.

2. A movement of the soil on which the water lies, an earthquake. It is evident that a shock given to a basin can put the water in oscillatory movement. In fact, it happens frequently. I will only recall the colossal transmission-waves in the Pacific Ocean on August 13, 1868, after the earthquake of Arica; that of May 9, 1877, at Iquique; and in earlier times, the earthquakes at Messina, 1783; at Port-Royal (Jamaica), 1692; at Callao, 1586, &c. If such enormous waves had taken place in a closed basin, as our lakes, it would probably have brought about an oscillatory movement of *seiches*. I could bring many facts to prove it; only one example, the earthquake of Lisbon (1755), was noticed in Switzerland and Germany, chiefly by the movements of the water of the lakes; the description of these movements recalls perfectly the *seiches*.

It is also theoretically probable that the shock given to the ground extends to the waters, and that an earthquake will produce *seiches* in a lake. Unfortunately the facts observed up to this time do not confirm this theoretical view. Since I established at Morges a self-registering limnimeter of the greatest sensitiveness, in March, 1876, six different earthquakes have been noticed in our country, and specially three earthquakes were felt at Morges itself—May 7 and November 29, 1876, and October 8, 1877. Not one of those six earthquakes has been traced by the self-registering limnimeter; not one has interrupted the rhythmic oscillation of the *seiches* which were taking place; not the smallest alteration of the curve has shown that the water had been acted upon in a peculiar manner; neither was the limnimeter of M. Ph. Plantamour, which was at work during the earthquake of October 8, 1877, influenced by that very severe shock. And yet our apparatus are extremely sensitive; when the lake is sufficiently calm my limnimeter can show the waves originated by a steamboat which passes 10–15 kilometres off the apparatus, or it registers the waves caused by a steamer which has passed by my observatory two or three hours before.

How can these contradictory facts be explained? On one hand, the earthquakes cause in many places enormous waves; on the other, three earthquakes strong enough to have awakened men out of their sleep, have not put in movement the most sensitive, always working, self-registering apparatus.

I suppose that the shocks of the earth do not transfer always the movement to the water; that only in a special direction of the shock a special intensity, a special duration, the water itself is put in movement and takes the rhythmic oscillation of the *seiches*. If I shake a basin the water is not always and necessarily put in oscillatory movement. I think it is the same for the *seiches*, and I believe that only certain movements of the earth cause the water of the lakes to move.

It is the point on which I seek an answer from naturalists who have more opportunity to observe the effects of earthquakes. I ask if each earthquake is accompanied by waves of the sea; if each shock of the same intensity is accompanied by waves of the same amplitude; if there are not differences between the different earthquakes; if some have the enormous waves of Iquique or Arica; if others are without those waves?

I should be very thankful to receive an answer to the above questions.

F. A. FOREL

Morges, Switzerland, January 24